

(1902)

Pleas, concerning the observation of *Easter*, registered in the *Phil. Transf.* 240, made this Calculation.

Rightly to understand the Rule in our Common-Prayer Books for finding *Easter*. Note,

1. That the 21st of *March*, in all but Leap-years, and in Leap years the 20th of *March*, was, at the time of the Council of *Nice*, when this Rule was made, the Vernal Equinox: Consequently,
2. That the 20th of *March* in Leap-years is the same as the 21st of *March* in common years.
3. That the Full Moon meant in this Rule is not to be found in our Almanacks, but by the Calendar of our Common-Prayer Books, where, in the first Column the Golden Number of every year is placed over against the day of the New Moon in every month of the year.
4. That the fourteenth day, including the first day of the Moon, is the Full Moon, and not the fifteenth, as Dr *Wallis* would have it in his Letter.

VII. *An Account of an Experiment made at a Meeting of the Royal Society at Gresham College, upon the Propagation of Sound in Condensed Air. Together with a Repetition of the same in the open Field, by Mr F. Hauksbee.*

A Bell being included in a Brass Recipient, and plac'd at one end of a Room, about 50 yards in length: At the other end of which stood some Gentlemen to observe the sound; which before any Air was intruded, the Bell upon shaking was heard at that distance, tho' not without diligent attention. Upon the Intrusion of one Atmosphere (begging leave to call it so) the Bell being shaken as before, the sound was very sensibly augmented;

two Atmospheres being Impell'd, and the Bell made to Ring, a notable Improvement of the Sound was then manifest. But upon the Intrusion of the 3d, 4th and 5th Atmospheres, the Bell being made to strike, the Sound seem'd not to be Propagated proportionably to the first and second ; which might happen by means of some Escapes of Air, or, which is much to the same purpose, the Valve, which should have hindred the Return of the Injected Air, did not hold so tight as I expected, or as it ought ; but that some of the Intruded Air would repass into the *Syphon*, by which means the Quantities suppos'd Injected were deficient. Another reason the latter Atmospheres did not propagate the Sound as the two first, is this, altho 25 Compressions of the *Syphon* are equal to the Natural Content of the Recipient, yet when the Air becomes pretty strongly Condens'd, as by the Intrusion of 4 or 5 Atmospheres, the remaining Air at every stroke which will lye between the Bottom of the *Embolus* and the Valve (altho but little) is at the same Density at the same time as the Air in the Recipient ; which, upon drawing up the force, will extend it self to supply such a space of the Cylinder as so much common Air ; and is so much short of what should be Injected at every stroke, 25 of which become equal to the Natural Content of the Receiver. Hence the Deficiencies of the Real Atmospheres or Quantities, by a certain number of Strokes may be very considerable, and to account for them most difficult ; and so I must leave them for the present, begging pardon for this digression. I conclude, Notwithstanding the Deficiencies are not known, yet at the end of the 5 (tho Imperfect) Atmospheres, the Bell being shaken as at first, it was the Opinion of a Gentleman then 50 yards distant, that the Sound seem'd to be almost as and as sensible as when it came to be made and expos'd in the open Air.

June the 9th, about 5 in the morning I repeated this Experiment in an open Field, known by the name of th.

the White Conduit, situate on the West of Islington, with much the like success as the former. Upon shaking the Bell before any Air was intruded, it was but just audible at 30 yards distance. Upon the Injection of one Atmosphere (begging leave as before to call it so) it became then as audible at 60 yards, as it was before at 30. A second being intruded, the Bell upon shaking might then be heard at 90 yards distance. But after that, tho' near 100 strokes of the forces were repeated, yet could it hardly be heard 20 yards farther. Which I attribute in a great measure to the Reasons before given. The Morning was very Misty, which caus'd a great Dew upon the Grafs. Little or no Wind stirring. And the silence requisite in making such an Experiment nicely, at last began to be interrupted by the sounds of the five a Clock Bells, and other Noises which joyn'd in Chorus with them from the City, contributing in some measure to the unsuccesfulnes of the latter part of the Experiment : Which still I hope to prosecute farther, not despairing of finding such a Gage as will shew the Certainty of the Quantities injected, without any danger or hazard in the Attempt.

*VIII. An Experiment made at a Meeting of the Royal Society,
touching the Diminution of Sound in Air rarefy'd. By Mr Fr.
Hauksbee.*

ABELL being included under a Receiver, which being shaken to make the Clapper strike, it was very observable that the Interposition of the Glass betwixt the Bell and the Ear, was a great obstruction to its Sound, notwithstanding it was audible at some good distance from it : But gradually withdrawing the Air, and making several Stops to shake the Bell at different Degrees of Rarefaction, the Diminution of the Sound at every Stop was very distinguishable. Till at last, when the Receiver was well exhausted of Air, the remains of Sound was then so little, that the best Ears could but just distinguish it : It appearing to them like a small shrill Sound as at a great remoteness. Upon suffering the Air to re enter (which was not done all at once, but by turns) 'twas easie to perceive the increase of Sound at the different times the Bell was made to ring : The Recipient being again replete with Air, the Sound then seem'd something more clear and audible than it did upon its first inclusion.